Estimating juvenile winter run abundance and life history characteristics at Chipps Island

Patricia Little Brandes

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0327: Estimating juvenile winter run abundance and life history characteristics at Chipps Island

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The overall rating for this study is very positive both from my personal reading and from the technical reviewers. The proposed study will be providing a very good genetic database of the winter run of juvenile salmon, thereby increasing our knowledge of the different races that are leaving the Central valley Delta during winter runs. The main PI is providing access to the raw samples that are then being sent to credible and proven experts for analysis. The general concern appears to be the paucity of detailed information on the sampling and analysis techniques that will be used. However, some of the reviewers have conducted their own background checks on the lab / consulting firm that the PI plans to use and their background knowledge and experience makes up for this deficiency in the proposal. One issue that was brought out by one of the reviewers was the ability for the PI to access and collect the raw samples in 2006 and 2007 given information from a another proposal that indicated that the sampling program on which the PI plans to piggy-back her sampling is uncertain at the moment. However, the PI does not give any indications that suggests that there may be difficulties obtaining samples in 2006 and 2007, therefore perhaps the project she plans to use differs from the one the reviewer read about in the other proposal. Regardless, the knowledge

obtained from another proposal should not outweigh knowledge given within the current proposal as the applicability and accuracy of this external information is uncertain. The overall rating for this proposal is very good to above average.

Additional Comments:

The overall rating for this study is very positive both from my personal reading and from the technical reviewers. The proposed study will be providing a very good genetic database of the winter run of juvenile salmon, thereby increasing our knowledge of the different races that are leaving the Central valley Delta during winter runs. The main PI is providing access to the raw samples that are then being sent to credible and proven experts for analysis. The general concern appears to be the paucity of detailed information on the sampling and analysis techniques that will be used. However, some of the reviewers have conducted their own background checks on the lab / consulting firm that the PI plans to use and their background knowledge and experience makes up for this deficiency in the proposal. One issue that was brought out by one of the reviewers was the ability for the PI to access and collect the raw samples in 2006 and 2007 given information from a another proposal that indicated that the sampling program on which the PI plans to piggy−back her sampling is uncertain at the moment. However, the PI does not give any indications that suggests that there may be difficulties obtaining samples in 2006 and 2007, therefore perhaps the project she plans to use differs from the one the reviewer read about in the other proposal. Regardless, the knowledge obtained from another proposal should not outweigh knowledge given within the current proposal as the applicability and accuracy of this external information is uncertain. The overall rating for this proposal is very good to above average.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Estimating Juvenile Winter Run Abundance and Life History Characteristics at Chipps Island

External reviews were thorough and generally positive about the proposal.

The panel concluded that developing the sampling design during the first year, rather than prior to starting the study, was a major problem. The panel would have liked to see the researchers work out the sampling design prior to submitting the proposal.

While the reviewers felt that the project would result in helpful information, the panel felt that the benefits of the study were not detailed enough in the proposal to warrent supporting this research program, as written. The statistical analysis methods were also lacking detail.

One reviewer expressed concern with regard to the costs that were being expended to employ a consulting firm, but others felt the project was "cost reasonable".

Rating: adequate

Technical Review #1

proposal title: Estimating juvenile winter run abundance and life history characteristics at Chipps Island

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The objectives and hypotheses are very well presented. In my opinion, however, the goal provided by the authors understates the value of the work proposed. Given the overall high quality of the proposal and the expertise of the principle investigators the likelihood of a successful outcome to the work supports a perhaps more lofty statement of the goal of this work.** **One minor comment: "Life history characteristics" should replace "scale patterns" in the Project Objective section and elsewhere in the proposal. A description of variability in scale patterns a tool used to interpret and describe variability in lafe history patterns. Not a major point.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Yes, the study is justified relative to existing knowledge. The proposal builds nicely on previous work and explicit requests for work by CALFED. The**

	conceptual framework for the proposal is adequately presented and compelling.
Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed and appropriate for meeting the objectives of the project. The authors clearly state the need for the work, the limitations of past approaches used to resolve the abundance of winter-run salmon in a mixed cohort, and the promise of newer technology now available. It seems very likely that the work as proposed will successfully address the specific objectives, by themselves important and timely, and, further, address other issues such as the ability to discriminate among other races.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach as described is thoroughly documented and technically feasible with a high probability of success. The authors have handily limited the scope of the project to winter-run chinook due to the existing knowledge of the degree to which genetic substructuring of the species can be resolved but they have clearly expressed their awareness and interest in increasing the value of DNA-based techniques.**

	Of particular value is the commitment to determining the rigor of the statistical design of the proposed work as the first step towards implementation. That is an element of much proposed work that gets much less attention than it deserves.
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Long−term monitoring, per se, is not a specific element of the proposal. However, of particular interest in this proposal is the commitment to reanalyzing past data related to patterns of migration of salmon juveniles past Chipps Island. Should that effort prove fruitful, the work will make a significant contribution to the value of that dataset and might reasonably be expected to contribute greatly to our understanding of long− and short−term variability in migration patterns and abundance estimates.**
Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Products of value are likely from this project. Still, I would recommend that the authors commit to a comprehensive completion report in addition to the annual reports and peer review publication(s). A completion report is likely to contain a larger volume of useful information than one or more peer−reviewed**

	articles, will appear in a more timely fashion, and will be more accessible to managers and other researchers than a set of annual reports. The production of a review and reinterpretation of the long history of past juvenile sampling efforts should be an important contribution in and of itself.
Rating	very good

Additional Comments

Comments	An important logistic aspect of the project is the availability of DNA and scale samples from the Chipps Island sampling program at no appreciable cost to the project. The proposal notes that the funding and commitment to perform the sampling is in place for 2005. However, the likelihood that sampling will occur in 2006 and 2007 is not made clear in the proposal. In another proposal I am reviewing (CWT fry/smolt releases for monitoring future changes in juvenile salmon survival in the delta) the certainty of the CVPIA B2 funding for Chipps Island sampling is described as uncertain for the future. That issue needs to be resolved.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The track record of the principal investigators contributing to this proposal is exemplary -- both in performing the preliminary work that is the foundation of the current proposal and copious other research projects.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget seems reasonable and adequate for the proposed work. However, in the additional comments section above I remarked that there appears to be some uncertainty related to the "outside" funding that will be used to obtain the juvenile samples in 2006 and 2007. That uncertainty must be resolved or contingency plans for alternate acquisition of samples developed. Trawl sampling is expensive.**
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is one of the better proposals I have reviewed. The coupling of DNA-based analyses with descriptions of life-history variation on a large geographic scale is an emerging technology with enormous potential for increasing our understanding of these creatures and the effect of their enviroment (and us) upon them.**
Rating	**excellent**

Technical Review #2

proposal title: Estimating juvenile winter run abundance and life history characteristics at Chipps Island

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goal of this proposed project is to genetically sample salmon to determine the proportional abundance of winter run Chinook salmon. Current methods of separating different salmon stocks depend on average differences in length that are not very reliable. The proposed method would allow better estimates of the abundance of various stocks. Additionally, the proposed study will include collections of scales from sampled fish to determine whether different stocks/ populations differ in growth rates, etc. This segment of the study strikes me as added on to make the whole seem more fundable. I have doubts that the scale portion of the project is viable– to be further discussed below.** **The main idea, that of exploring the utility of genetic markers to differentiate various races/stocks of winter run Chinook salmon has considerable appeal.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study appears to be well justified relative to existing information, although the referenced literature is slight. Nevertheless, the methods for genetic analysis are sufficiently documented to allow an assessment of the viability of this approach. The same cannot be said for the scale sampling.** **This is a full study and should be funded as such.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The proposed study will yield information of general use to fish managers and scientists. Nothing novel is likely to come of this study, unless the proportions of winter run Chinook are dramatically different that those of previous studies, which were based on length differences. Additionally, although I am skeptical of the scale component, an interesting result might obtain.**
Rating	**excellent**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The genetic sampling is sufficiently documented and is viable. It will yield results. The contractors identified by the PI appear to be competent for successful completion of the project. However, it is clear that the PI has very little understanding of this subject, as reflect by awkward phrasings through**

	out. In particular, the PI has no understanding of the statistics relevant to the genetic sampling. She repeatedly refers to the power of the technique, when in actuality she should be referring to the assignment success of the technique. The PI is the weak link in this study. **Regarding the scale sampling, I would have liked to see a literature reference to the use of circuli for aging fish. Is it possible? Potentially. However, this generally is a very imprecise method and is poorly validated. Further, how this data will be linked to "life history" characters is not elucidated. The PI does not address how differences among sites in water temperature, which will affect growth and therefore apparent age based on circuli, will be evaluated. I see this study segment as something thrown in as filler and its presence/absence is basically irrelevant to my commentary on this proposal.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**There is no monitoring component to this project although sample collection will be linked to existing sampling programs.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	

	The potential of gaining a better understanding of proportional abundance of different races of salmon warrents consideration. Study results will be of general interest. **The PI suggests one publication will result from the study. This is a rather modest return on the funds to be invested in this project. I see an obvious publication from the genetic studies. But, what? Is the PI conceding the scale study segment is without merit?**
Rating	**very good**

Additional Comments

Comments	**I have no dog in this fight, except as a taxpayer. This is quite possibly the best of the four proposals I evaluated in terms of producing something of interest. And I'm a warmwater biologist. This being said, I have a general concern about this study. It appears the PI is interested in this work and has lined up persons to perform it– no matter the cost– because, what the hey, its all a crap shoot. I would like to see a tighter budget. The funds allocated to the consulting firm are excessive. I worked for one once and I still think this.** **Goal 5 of the research– estimate trawl efficiency– also strikes me as something thrown in. This is a general feature of all salmonid proposals I saw in this program. Offer to look at existing data. In my contract and granting circles, this is upfront, proof of concept stuff.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The staff appears to be capable of completing this research. The PI appears to function only as a front person. All work, including report writing and publication, are contracted out.** **It should be noted that the proposal suggests that (1) the proposed study will basically consume all time/resources of the genetics lab to be used and (2) that that lab is also involved in submission of other proposals to this program. I don't care what football coaches say, no one can give more than 100%. This is an important concern, reflecting on the credibility of that lab and this program.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is a bit steep. Mostly because of extensive use of a consulting firm. Given that they have charged for report writing, the time they then allocate for publication is gratuitous.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**Interesting idea, only one study segment– genetic analysis of races– is of interest. The scale sampling is speculative and should be supported by preliminary data. Similarly, the analyses of existing data for estimating trawl efficiency should have been proved to work (i.e., a little up front work). I note the statistical design for selection of samples is yet to be completed– it really won't take long to do this (I used to do this stuff for a living). Again nothing is**

	given up front. I'd like to see the design. For the funds requested, I don't think this is an unreasonable request.
Rating	**good**

Technical Review #3

proposal title: Estimating juvenile winter run abundance and life history characteristics at Chipps Island

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**There are two hypotheses stated in this proposal which propose the questions : How many winter run juvenile Chinook Salmon leave the Central Valley Delta each year and can scale pattern differences (meristics) differentiate juveniles derved from distinct genetic stocks. The goals and objectives of the proposed work clearly relate to these two hypotheses and are clearly delineated in the objectives. Yearly records from past sampling at Chipps Island will provide an outstanding database for the statisical modeling programs incorporating new advances in DNA microsatellite technology and improved scale meristics. This goals and objectives are well stated considering the ecological importance of Chinook salmon to the central valley as they are the "lynchpin" fish species and. I explored maps of the central valley and delta area to assist in my review and noted the human activities, development, and population levels in this area. It is clear that the river/estruary system is greatly endangered. The pre-existing fisheries database is vitally important for the development of improved models using more technically advanced methods which should better define genetic subpopulations in this system with a reduced error rate.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**This project is ready for full scale implementation. I performed a literature search in regards to the proposed work by the laboratory of Dr. M. Banks and S.P. Cramer and Associates being that their parts of the project are not only critical to the goals and objectives but that their technical methodologies are "ready to go". Though the previous database is extensive over many years of sampling at Chipps Island, genetic assignments based on phenotype and meristics has been a a substantial "grey area" of fisheries science. The incorporation of a significant array of genomic DNA markers that can be assessed via a high throughput procedures is needed right away. Coupled with improvements that yield highly detailed scale meristic measures a significant new contribution to understanding the dynamics of winter run Chinook Salmon can be delivered. The conceptual models are clearly stated and presented in a proper order. Considerabla date will be expected from 3000 samples per year to re−evaluate past modeling efforts and deliver a far more precise picture of the winter run (and other season runs) of salmon. Obviously, the statistical analysis and population biology modeling, as expected, will be improved. Figure 1 and 2 in the proposal clearly demonstrate such modeling applications. I am an expert in DNA based genotyping technologies and am greatly enthused about the knowledge gains possible from using DNA microsatellite markers in this study.**

Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed and presented. The proposal will incorporate past years data and clearly states the sampling plan at Chipps Island, disposition of samples to participating laboratories, DNA and scale analyses, and data analyses/modeling. I would have liked to have a table (or two) and/or paragraph that presented information delinearating the known genetic strains/stocks and the proposed genetic markers to be used, as genetics reviewers would be greaty interested in exploring this information. There is no way to shortcut the need for a large sample size for DNA and scale work and this is well addressed. Multiple DNA microsatellite, RAPD, RFLP, and AFLP genotypic markers are the preferred new approach to population genetic studies. I have used these exclusively in my studies of Largemoutrh Bass population genetics to the extent that I can easily distinguish Florida Large mouth bass hathchery stock from non Florida stock taken from Florida stocked Southern USA bodies of water. This was necessary as only a single and frequently unreliable allozyme arker was historically used. I am also familiar with advances in scale meristics and am confident that Cramer and Associates are using the most advanced methodologies to measure life history parameters. Scale pattern studies have advanced considerably and coupled to DNA based population genotyping should provide a whole new view of salmom population genetics and population dynasmics. For site sampling, DNA analyses, and scale pattern studies The authors have presented sufficient supporting reference literature**

	to justify their scientific approach. An additional literature and background exploration by myself reinforces this interpretation. The use of the data for statistical and modeling work by Brandes and co−investigators is based upon considerable experience in fisheries data analyses. Indeed, novel and new data sets will be generated that can be expanded to include additional genetic markers and meristic measurements. Better models of salmon life history and stock/strain measures wil be available especially when coupled to other studies of salmon in the region. Comparison to fry−smolt data incorporating information from Red Bluff will be greatly informative. Overall the approach should deliver a far more refined and accurate information set on endangered winter run chinook salmon in the Central Valley. This is very important in light of the great threat to natural populations on the west coast.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**I do feel some genetic strain/stock information plus a table of the specific genetic loci to be screened would be more helpful to the proposal. The proposal does provide sufficient details and references to the methodologies that will be used. Documentation is nearly complete otherwise. The sampling plan and the following DNA and scale pattern analysis is ambitious but necessary at this time in the development of this science. Since the sampling is ongoing it should not be difficult to obtain the fin clips for DNA analyses and scales for meristics (example: I do trust that at least three scales and two fin clips will be taken from each fish caught). Sampling, distribution, and sample processing should follow a a detailed QA/QC and GLP format. Are participants required to supply such**

	plans? The DNa microsatellite procedures and scale meristics are documented, established, and validated for ready implementation as these are accepted in their respective scientific fields. This should "work" and I expect the project to be sucessful. Once the data is obtained and subjected to statistical analyses and the modeling performed we should have a very interesting and somewhat different yet more accurate picture as pertains to the hypotheses and goals of this study.
Rating	**excellent**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Considering that a significant sector of this study involves field sampling, partitioning of samples to participating labs, and sample analyses, the use of a QA/QC and GLP monitoring is most important. The critical methodologies for DNA microsatellite and scale meristic analyses are well developed and standardized by the principal laboratories. This is, aptly speaking, a large scale population genetics/population biology study that requires a considerable data input for modeling analyses of the winter run strain/stock analyses and fry/smolt survival. This study will require excellent record keeping in order to follow sample collection, distribution, processing, and analyses. Obviously here the traditional lab experimental design setup would only apply to the previously performed research that validated the DNA and scale analyses to be used. The statistical analysis and subsequent modeling will incorporate the new information with past data and related adjunct studies. This is indeed the more creative component of the proposed study that will build upon an established set of statictical and modeling programs.**

Rating	**very good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The DNA microsatellite and scale analysis proceedures seem well established and as a product could be extended to studies on other fish species not only in fisheries but other areas of biology. Personally I would be interested in DNA microsatllite applications in regards to my own fish genetics work. Digital scale meristic analysis is used more extensively now hence it is a well developed product. The most valuable product will clearly be the development of a population genetics/biology tool that has low error rates in dscriminating salmon stocks/strains that can be employed on a large scale for variuos fisheries studies. A more precise genotyping of natural populations and hatchery stock would also be highly valuble. I am confident that a more correct estimate of winter run smolt populations can be extracted from this study and offers advances in data management as well predictive modeling on the natural history of this critical fish species**
Rating	**excellent**

Additional Comments

Comments	**There are plans for obtaining lare numbers of samples over three years. I do encourage a brief yet concise QA/QC statement be added pertaining to sample**

	disposition and labeling. Will fish length and weight be recorded? Will eplicate samples (scales or fin clips) be taken?

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The USFWS lab of P. Brandes has considerable experience and a great track record in salmon fisheries population biology, statistical analysis, and modeling. Figure 1 in the proposal provides a good example of their capabilities. The laboratory of Dr. Banks at Oregon State is fully cabable of handling 3000 DNA samples per year, though ambitious, the high throughput ability to screen 12 DNA microsatellites markers per sample should go well. Dr. Banks has considerable expertise in this area and I am familiar with his excellent DNA based studies. DNA microsatellites are excellent DNA genotyping markers. Considerable effort was put into developing and validating these markers. S.P. Cramer and associates is considered highly expert in meristic studies using digital analysis to greatly up the throughput of what was once historically a painstaking and labor intensive analyses. The specialized equipment needed in all the participants lab's is in place. This also includes their sample handling, sample processing, and data management capabilities. I like the modeling capabilities. This area is probably their greatest strength relative to the proposal as a whole.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**I have examined the budget and find it "cost" reasonable. The biggest cost advantage here is the ongoing yearly sampling at Chipps Island. Field sampling can be a logistical nightmare and labor intensive. I know this from my numerous field sampling excursions. The sampling regimen is easily "piggy backed" onto the already established setup. This is a great convenience. The DNA microsatellite sample analysis of 300 X 3 = 9000 samples over three years for 12 microsatellite markers/sample comes to about $36 per sample. This is very reasonable. Scale analysis of 9000 samples comes to about $12 per sample, again, very reasonable. The data management, statistical analysis and variuos modelig and life history/survval goals are very reasonable. With an extensive infrastructure already in place. The project is ready to implement.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**Overall, this is a very good – excellent (mostly excellent) proposal. I recommend funding based upon the demonstration of an infrastructure being in place, established analytical procedures, cost effective operation, and a very experienced team of scientists. I would have preferred a little more information on the genetic markers, specifically, a descriptive table showing some information on their genomics as well as a summary table showing Chinook Salmonstocks/strains identification relative to the 12 DNA microsatellite markers to be used. I am confident that they are validated and ready to be applied in this work.**

	Similarly, some additional info on the scaly meristics would be good. Again, a table or figure would be helpful. I have a great interest in DNA microsatellite applications in fish genetics so I wish the proposers good luck on this. The P.I. has past experience with CALFED and the goals and objectives critically attend to the concerns of the funding agency.
Rating	**excellent**